

the soldier's point of view, the difference is all-important; for he regards a home established by an agency outside his regiment as a charity, whereas his selection by his regiment as an inmate of one belonging to that regiment is considered a privilege and an honour.

Correspondence.

"Audi alteram partem."

THE INFAMOUS ACCUSATION AGAINST DR. A. E. JONES.

To the Editors of THE LANCET.

SIRS,—In your issue of May 5th you commented sympathetically upon the conclusion of the hearing of the charge against Dr. A. E. Jones of a-sault upon certain mentally defective children during his official examination of them at the London County Council day-school which they attend. Completely successful as was the result of the case, it was, of course, not accomplished without considerable expense which falls entirely upon Dr. Jones. Baseless charges of this kind are unfortunately only too well known to the profession which has always readily shown practical sympathy with the victims of an injustice to which all its members are equally exposed. This case, moreover, displays special features which call for more than usually energetic protest if the status of the profession is not to be seriously jeopardised. All are familiar with the class of persons by whom such false charges are usually brought, but this is the first time that an important public body has supported an accusation of this type which originated with mentally defective children, an accusation, moreover, which the magistrate dismissed without listening to the defence.

It is felt that a subscription to defray Dr. Jones's legal expenses (which amount to £180) would be the most satisfactory way of expressing the sympathy of the profession and of its intention to meet such attacks as a united body. A small committee, consisting of the undersigned, has been organised for this purpose and Sir Victor Horsley has consented to act as treasurer. Bearer cheques may be made payable to, and crossed, Barclay and Co., and sent to Messrs. Barclay and Co., 54, Lombard-street, E.C., or to Sir Victor Horsley, 25, Cavendish-square, London, W.

We are, Sirs, yours faithfully,

S. H. BOWN, CHARLES BOLTON,
P. MAYNARD HEATH, C. WYNN WIRGMAN.

May 16th, 1906.

The following subscriptions are announced:—

	£	s.	d.		£	s.	d.
Mr. Arthur E. J. Barker	5	5	0	Dr. Herbert R. Spencer...	5	5	0
Dr. J. Rose Bradford ...	5	5	0	Dr. G. F. Blacker ...	3	3	0
Dr. Dudley W. Buxton ...	5	5	0	Mr. Rupert T. H. Bucknall	2	2	0
Sir Victor Horsley ...	5	5	0	Mr. Raymond Johnson ...	2	2	0
Dr. Sidney H. C. Martin	5	5	0	Mr. M. D. Eder ...	1	1	0
Mr. Bilton Pollard ...	5	5	0	Mr. E. S. Weymouth ...	1	1	0

CASEIN AND CASEINOGEN.

To the Editors of THE LANCET.

SIRS,—It is with mingled feelings of pleasure and amazement that I have read the letter of Professor W. D. Halliburton in reply to my protest; the pleasure comes from finding that some of the reasons given for the nomenclature are rational, concisely stated, and ones which I can accept. I am amazed, however, that Professor Halliburton should derive caseinogen from the Latin; the root of *genero* is not *gen*, but *gener*, and I think he will pardon me for not having recognised the Latin origin. Casein is not a Latin word, but judging from the gender of the French and Italian the Latinised form would be *caseina* and a more correct name would be "*caserna-generin*" (I have given the word the termination "in" to indicate its proteid nature). Caseinogen as a word derived from the Latin is but little less objectionable than as a hybrid.

I am still more amazed that Professor Halliburton calls the instance of the confusion that arises from his nomenclature which I pointed out in Dr. Gustav Mann's book an obvious press error; in the index Dr. Mann gives 50 references to casein and two to caseinogen (the word, however, occurs many times in the text in these two references); one and one only of the

casein references is to casein in Professor Halliburton's sense; in two of them (pp. 70 and 353) the word caseinogen only and not casein is mentioned in the text, and in the bulk of, if not in all, the other 47 references the word casein is used obviously in the sense of caseinogen. One instance especially may be quoted in full (p. 378): "Fibrinogen, myosin, and casein assume a firm state of aggregation when acted on by certain ferments." This passage clearly shows not only is Professor Halliburton's nomenclature departed from in "casein" but in "myosin" as well, and it cannot be an "obvious press error" when two words are given in the old form. It is the tacit sub-conscious revolt of a scientific mind against what I have ventured to term a philologically barbarous nomenclature. I am again amazed that Professor Halliburton should speak of "antique terminology." Is he aware that in the index of the *Journal of the Chemical Society* for 1905, quite lately published, "caseinogen" does not occur, but the proteid to which he gives this name is called "casein"? Does he know that in the abstracts of the Chemical Society, except in those made by himself, the word "casein" has been used this year? Has he forgotten that in the January number (vol. i., p. 56) he wrote, "A comparison of solutions of casein and para-casein (or as they are usually called in English caseinogen and casein respectively)....."? And is not this a tacit admission that "caseinogen" is still largely called "casein" and "casein" "para-casein"? Does not Dr. Thorpe, an ex president of the Chemical Society and chief Government chemist, use "casein" (*Transactions of the Chemical Society*, 1905, pp. 214, 216) only last year?

Professor Halliburton asks if I would propose to drop the word "fibrinogen" and call it fibrin; my answer is that fibrin should be retained for the proteid to which it is at present applied; it has priority and is universally accepted. I should like fibrinogen dropped or modified. I am glad to hear that a joint committee of the Chemical and Physiological Societies has been appointed to consider proteid nomenclature but deplore their recommendation; I can only think that they have not had the case against the use of caseinogen put before them. I may say that I do not object to "caseinogenate" on account of its length, I cannot pronounce it euphoniously.

Finally, I would like to recapitulate the chief objections against "caseinogen": 1. It is "philologically barbarous." 2. Casein has priority. 3. It is far from universally used even in England and still less so in America. 4. The German "kasein," the French "caséine," and the Italian "caseina" are generally used and the natural word which occurs for the translation of these is "casein." (The confusion which exists is due to 3 and 4.) 5. It departs from the well-established rule of chemical nomenclature that similar compounds have the same termination. For this very proper reason the old terms "caseine" and "albumen" have been changed to "casein" and "albumin."

It would be quite easy to retain the advantages pointed out by Professor Halliburton and at the same time to abandon "caseinogen."

I am, Sirs, yours faithfully,

H. DROOP RICHMOND.

Woodfield-road, Ealing, W., May 12th, 1906.

To the Editors of THE LANCET.

SIRS,—As Mr. H. Droop Richmond has courteously sent me a copy of the letter on this question which he is forwarding to you for publication I am enabled forthwith to add a rejoinder. It shall be very brief and, so far as I am concerned, final, for I am well aware that your space is too valuable for discussions of this nature. I will only say that I am not convinced by Mr. Richmond's arguments and will refer those interested in the question to my letter of last week, as I find myself unable to state my own views any more clearly than I have put them there. The only statement there made which I should like to amend relates to what I said about Dr. Gustav Mann's book. The particular case of confusion in the use of the two words under discussion, and which appeared to me to be a printer's error, is not, as Mr. Richmond correctly states, the only instance where the words are wrongly employed. I admit that authors as well as printers are occasionally careless, but I fail to see in that any reason for adopting Mr. Richmond's views. Physiologists in this country have practically unanimously accepted the words caseinogen and casein in the sense in which I have urged they should be used. I feel hopeful now that Mr. Richmond

admits some of my reasons to be rational and acceptable to him that chemists also, when the "tacit subconscious revolt of their minds" has subsided, will fall into line also. When we have set our own house in order it will be time to try to persuade foreign nations to follow suit, and it is the intention of the proteid nomenclature committee, to which I referred last week, to obtain the opinion of other countries regarding their proposals before putting them forward in any authoritative manner.

I am, Sirs, yours faithfully,

W. D. HALLIBURTON.

King's College, London, May 14th, 1906.

THE TRANSMISSIBILITY OF GOITRE.

To the Editors of THE LANCET.

SIRS,—The following history seems interesting enough for publication. A perfectly healthy family came from a village near the Exe and settled here some 36 years ago. No member of the family, so far as they can tell, ever suffered from any enlargement of the neck previously to their coming here. There were three daughters. The youngest went to work at a farm in this village at 15 years of age and rapidly developed goitre. (Another help at the same farm has recently also developed goitre very rapidly.) She left the farm and married and the goitre has persisted but has not increased. Her first eight children were born healthy, the ninth had well-marked goitre and died in infancy from whooping cough, and the tenth, born last month, has also well-marked goitre. All the children were suckled and she is sure that the last two never drank any water at any time. The well water here is extremely hard and goitre was, it seems, very common before the present supply was instituted. It is practically extinct now and I know of no recently developed case except the one at the farm mentioned, which is one of the few places where they still drink well water.

The history is interesting, for it is impossible to doubt that she acquired goitre from a definite source and had apparently no hereditary tendency. After eighteen years, while no longer exposed to the poison, she seems to have communicated the complaint to her offspring. During the last two years she has not drunk any unboiled water by my advice and she has not drunk well water for several years. My books of reference say that goitre is occasionally hereditary and may occur in infancy, but I can find no case similar to the above, which seems to militate against the axiom which tells us that "Acquired peculiarities are not hereditary." It almost suggests that goitre poison is like syphilitic poison and may be acquired and passed on to the offspring.

I am, Sirs, yours faithfully,

O. CLAYTON JONES, M.B. Oxon.

Silverton, Devon, May 7th, 1906.

ANTERIOR OR POSTERIOR GASTRO-JEJUNOSTOMY?

To the Editors of THE LANCET.

SIRS,—In THE LANCET of May 12th, p. 1347, Mr. Edward Ward makes a claim, on Mr. H. Littlewood's behalf, to priority in two "essential features" of the operations of gastro-enterostomy and of intestinal anastomosis, "the use of rubber sheathed Doyen's clamps" and "the excision of elliptical portions of mucous membrane from each organ."

First, as to the use of the clamps. In Mr. Littlewood's paper to which Mr. Ward refers the mention of the clamps is merely incidental. The title of the paper—and it is important to note it—is "Intestinal Suture by Means of Continuous Catgut Stitch, and Excision of the Mucous Membrane." In his preamble Mr. Littlewood, after mentioning some of the mechanical aids used in affecting anastomoses, writes: "At the present time many surgeons have reverted to the earlier practice and have abandoned the use of any special apparatus, and I am amongst those" (implying obviously that he is only one among many) "who believe that all accessory apparatus for intestinal suture will ultimately be discarded and that we shall rely on our fingers, the forceps, and needles and sutures." There is not one word here or in any part of the paper which suggests that Mr. Littlewood considered that he was introducing any innovation in the matter of forceps. Mr. Littlewood himself, therefore, makes no claim whatever in this paper for any priority in the use of clamps in either stomach or intestinal work. Had he done so his claim could not have been substantiated. The use of the clamps in intestinal work was at

least 20 years old when Mr. Littlewood wrote. Kocher in 1880¹ describes and illustrates with four figures his method of intestinal resection in which clamps were used. Mr. Thomas Smith (now Sir Thomas Smith) in THE LANCET of Nov. 18th, 1893, p. 1230, referring to an intestinal clamp described in THE LANCET of Sept. 30th, 1893, p. 813, by Mr. W. A. Lane, drew attention to a clamp forceps "which has a more extended application in abdominal operations." "These forceps," he wrote, "have curved, flat blades with a parallel grip and they are made of various sizes; they can be applied to the stomach, gall-bladder, cæcum, intestine, or any cyst wall. Their purpose is to occlude the cavity on the convex side of the clamp and to isolate the part included within the blades during the application of sutures, whether it is for closing a wound or for attaching the part to the abdominal wall. The blades can be sheathed with rubber tubing or used naked." Doyen, whose forceps Mr. Littlewood used, described his *pince à mors élastique* and illustrated their use in stomach surgery in his work, "Traitement Chirurgical des Affections de l'Estomac et du Duodenum," in 1895 (pp. 294, 302, and 303). These references will, I think, suffice to show that the use of clamps was of much older date than 1900 and that Mr. Littlewood has no claim to priority in their use either in stomach or intestinal work. If Mr. Ward had been content to claim for Mr. Littlewood that he used the clamps before any other surgeon in Leeds I should have agreed with him entirely, as I have previously told Mr. Littlewood there can be no doubt whatever about that.

The second point is concerned with the question of the excision of the mucous membrane in anastomotic operations. Mr. Littlewood considered this, as the title of his paper shows, an essential and original part of his procedure. The paper which Mr. Littlewood read was published in THE LANCET of June 29th, 1901, p. 1817. My publication of a paper read before the Mirfield Medical Society appeared in the *British Medical Journal* of Dec. 8th, 1900, p. 1631. In that paper I wrote: "An incision two inches to two and a half inches long is made through the serous and sub-mucous coats only; as they are divided they retract and an ellipse of mucous membrane over half an inch wide in the centre and narrowing off at each end pouts into the wound. Two pairs of nibbed forceps, one towards each end, grip and pull upon the ellipse of mucous membrane, which is then excised."

When Mr. Littlewood read his paper in Leeds I joined in the discussion. He will, I think, remember that I then said I had practised the excision of the mucosa in the way I have described and in my remarks I attributed my thought of this procedure to the "stamping out" process of the Murphy button, which removes a portion of the apposed viscera and so forms an opening in place of a slit. I have, in conversation, since told Mr. Littlewood that I believed that in this matter the priority rested with me. In the first edition of "Diseases of the Stomach," written by Mr. Mayo Robson and myself, I devoted a paragraph (p. 273) to the question of the removal of the mucous membrane in gastro-enterostomy. I put my own opinion and practice first and Mr. Littlewood's second, because I thought then that this was the order of precedence. In later papers I have not hesitated to describe this procedure as due to my own thought and as owing its origin, in my own mind, to the use of the Murphy button.² Obviously, then, when this claim for Mr. Littlewood's priority is made it is a question of ascertainable fact which is at stake; it is not a question of opinion.

The evidence I offer is this. The first occasion upon which I performed the excision of the mucosa was on June 1st, 1900,³ the second was on June 6th, 1900, a patient of Dr. H. J. Clarke of Doncaster. This case is not included with the one just named because at the time I believed the condition to be malignant. It is, however, included in my "Report" to the British Medical Association last July, a copy of which I sent to THE LANCET (p. 87).⁴ I have changed the case from the "malignant" to the "simple" series, because the patient is alive and in perfect health at the present moment. The third case was on August 1st, 1900.⁵ These cases have been on record unchallenged now for over five years and during that time I have repeatedly expressed my belief that

¹ Centralblatt für Chirurgie, 1880, No. 29, pp. 465-469.

² Abdominal Operations, p. 157.

³ See Brit. Med. Jour., vol. ii., 1900, p. 1634.

⁴ THE LANCET, July 29th, 1905, p. 326.

⁵ See also Brit. Med. Jour., vol. ii., 1900, p. 1634.